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PROCEEDINGS

OF THE

BIOLOGICAL SOCIETY OF WASHINGTON

RECOLLECTIONS OF THE EARLY DAYS OF THE BIOLOGICAL SOCIETY.[1]

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In 1880 the workers of forty years before seemed to us almost prehistoric. I wonder if the workers of 1880 seem equally almost prehistoric to the young men of to-day. If reverence for elders has not entirely gone from the modern world (I know it has very largely) I can imagine that you look upon the founders of the Biological Society of Washington with at least a touch of that mental attribute which we used to call reverence or think of them perhaps as rather interesting old fossils. But as I look back the men who founded this society were very much like the men who compose it to-day. Even the first president, Dr. Theodore Gill, who was looked upon then as a man of extremely mature years, and who possessed a knowledge that only comes with long years, was in reality only 42 years old; and most of the others were in their twenties and thirties.

In 1880 the great concentration of scientific men in Washington was just beginning, and the great specialization was already making its appearance. Boston was still the scientific center of the United States, and I believe that not even the far-sighted Baird could have foreseen what we have all seen of the development of science under the governmental institutions in this city. The Philosophical Society had reached its destined repletion and was beginning to crack. The Biological Society was one of the very first of its children to leave the parental nest.

Already the young men who founded the new society have grown old and died, and I believe there are only two of them who are still active members of the Society in Washington—E. A. Schwarz and myself—curiously enough, both entomologists.

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I have elsewhere spoken of the coordination between the study of entomology and longevity, and this is perhaps another instance.

Speaking of entomology, the first meeting of this society was held in the house of an entomologist, the late Doctor Riley, but the first paper read before the society was by an icthyologist, the late Dr. Tarleton H. Bean.

When the society was founded I was a youngster of twenty-two. I had been elected a member of the Philosophical Society, but the secretary had forgotten to notify me of my election. I was an assistant to Prof. J. H. Comstock, at that time entomologist of the Department of Agriculture with a force consisting of Theodore Pergande, of a superannuated negro messenger with a taste for alcoholic preservatives, and myself. One day Dr. G. Brown Goode, young, filled with energy, and of charming presence and manner, called at our office and invited Professor Comstock and me to join in the movement to found the new society. Thus I became one of the original members.

In those days scientific men not only quarrelled (which, of course, they don't do any longer) but they absorbed the work of their assistants in the most extraordinary manner, some of them publicly defending this course and considering it entirely ethical. Looking over the proceedings of the society in the early years, we find no printed records of quarrels, but to men with good memories the titles and the brief printed statements recall many things which were never recorded in print and which it is lucky, probably, were never recorded in print. Thus, at the May 6, 1881, meeting Professor Comstock read a paper on scale insects. The record shows that on May 20th Doctor Riley discussed this paper and was replied to by Comstock. Just what they said has been lost. *Requiescat in pace!*

I attended very few of the early meetings. My evenings were otherwise spent—not so profitably perhaps, but more pleasantly to one in his early twenties. Undoubtedly I would have been a better and broader biologist if I had religiously attended all of the meetings, but I would not have had half as much fun, and probably I gained something elsewhere which has been of equal use to me in after life. This is heretical, but susceptible of argument.

In 1884 Doctor Riley (or rather one of his assistants—Web-

ster) made an important discovery, which included not only proof of a phytophagic habit on the part of an Hymenopterous parasite but established an alternation of generations with the species. Riley read a paper on the subject before the Biological Society, and I attended at his special request. The paper was couched in technical terms; its significance was not in the least understood by the audience, and I made my first speech before the society, under the head of discussion, by attempting to put into simple words of one syllable adapted to the limited understanding of botanists and ornithologists and mammalogists and paleontologists the fundamental biological importance of what Riley, in compliment to the wise physiognomies of his audience, had couched in polysyllabic terms of classical etymology.

The next time I was on my feet before the society, March 19, 1887, I read a paper of my own about a little aquatic insect of the genus Hydropsyche which makes webs under the water in Rock Creek and catches in these webs the larvae of Simulium, the adults of which are called buffalo gnats, or black flies, as well as other insects. That meeting had a greater interest than this personal one to me, since Alfred Russel Wallace was present and discussed this particular paper, expressing his astonishment that there existed forms which spun silk under water. Wallace was in Washington several weeks at that time, and attended more than one meeting of the society. His interests were very broad. He discussed from his wealth of experience, all aspects of biology. Unfortunately, he went into other things as well. He had already become a spiritualist, and his credulity regarding the acts of the most transparent charlatans almost destroyed the scientific idol which I had in a way worshiped since I first read "The Malay Archipelago."

This talk is not historical; it is simply gossipy and reminiscential, and it will necessarily be brief.

I wonder if some of those meetings would have seemed as extraordinary to you as they seemed to us. The story has often been told of how the society spent one whole meeting in discussing the position of the tail of the extinct Steller's sea cow, and then followed it two weeks later with another evening on the same topic, and then two weeks later with a third, the last of these three meetings continuing the discussion by special vote until half past ten! There you see were six and a half mortal

hours given up to a most intense discussion by Elliott, True, Coues and Gill on a point which seems not to have the slightest practical value to the world and which was finally settled in three minutes by Doctor Dall when, at the meeting of April 19, 1890 (years later), he exhibited a map drawn by a member of the Behring Expedition on the margin of which was a sketch from nature showing the tail in the position in which it was said to be by some one or more of the disputants, I don't know which.

There is another subject that came up for discussion several times in the early years of the society, and that is as to whether the turkey buzzard finds its food by sight or by an extraordinary sense of smell. The first time, I think, that this subject was brought up was in the discussion of a paper entitled "Notes Relative to the Sense of Smell in the Turkey Buzzard," read by Mr. C. L. Hopkins at the meeting of December 17, 1887. As I recollect, Mr. Hopkins was decidedly of the opinion that buzzards find their food by means of some extraordinary sense of smell; but there was a long and rather heated discussion, with the preponderance with Mr. Hopkins. Either he or one of the other speakers, I remember, told the story of some carrion completely hidden from view by a shed or something of the sort which attracted buzzards from great distances. But after many remarks, the following story was told of observations made by the late Dr. Otto Lugger. It seems that at a point on the beach of the lower Potomac, or it may have been Chesapeake Bay, there was a bluff a bit from the beach, and a fence running along the bluff. The farmers had the custom of collecting the useless fish, alewives and others, stranded on the beach or discarded from seines, carrying them up the bluff and putting them in a trench behind the fence, covering them with a sprinkling of earth, and eventually using them to fertilize their fields. Now, according to Lugger, buzzards would alight on the fence, always facing the beach, and would stand there for hours watching the shore for food. In the meantime the stench from the decaying fish ten feet behind them was overpowering; yet they sat there in their ignorance waiting for food on the beach totally unconscious of the hundreds of pounds of deliciously decaying food immediately behind them. After this story was told the argument stoppped; not another word was said, and the society ad-

journed. Did it settle the question? Or was the whole story an invention of the humorously minded Lugger?

At the meeting of April 30, 1887, the late Dr. J. H. Kidder exhibited some specimens, among them a round ball the size of one's fist and which was evidently composed of vegetable fibers and fragments, and (I imagine maliciously) gave no information about it except that it had been found on a shelf in the National Museum. And then the members began to guess. The most extraordinary theories were put forth. Van Deman, I remember, even thought that it might be one of the balls of hair from the stomach of a horse. After all sorts of theories had been advanced, Doctor Kidder stated that it had been taken from the shallow waters near the shore of one of the alkaline lakes of the West and that it consisted of fragments of aquatic plants which had been partially eaten by the larvae of the Ephydrid flies which inhabit these lakes and that the balls had been formed by wave action. I never knew whether he was right or not.

At intervals almost periodic there has arisen a discussion as to whether the flying fish flies. It may be of interest to know that this question was first brought up at the meeting of May 14, 1887, and that hot discussion followed in which W. B. Barrows, Admiral (then Engineer) Baird, Lucas, Goode, Hallock, Dall and Riley took part. Of course every one knows now that flying fish *can't* fly, just as every one now knows that flying fish *do* fly, and that the difference between the "can't" and the "do" depends entirely upon the definition of the verb *to fly!*

The decade from 1880 to 1890 marks what will possibly be known to history as the Gilbert and Sullivan era. Gilbertian expressions were quoted everywhere, and the charming jingle which began "The flowers that bloom in the spring tra la la have nothing to do with the case" may have suggested to that profound sociologist and eminent paleo-botanist, Lester F. Ward, the title of a paper which he read February 8, 1890, "The Flowers that Bloom in the Winter." In spite of the Gilbertian insignificance of the flowers of spring, Professor Ward's comments on the flowers that bloomed in the extraordinary winter of 1890 will be found of especial interest this year if the Washington botanists took the trouble to list the plants that flowered during the still more remarkable winter of 1918–19; but, as the botan-

ists now have their own society and have little to do any more with the Biological Society, how are we others to know?

In the spring of 1893 (meeting of April 22) O. F. Cook, then recently back from Liberia, read a paper on the natural history of that interesting colony of expatriated Afro-Americans. In talking of the termites, he referred to the fact that the queen lays eggs at the rate of about eight hundred thousand or more a day. This happened to be, I think, the only meeting in the history of the society which was attended by the well-known chemist, Dr. H. W. Wiley. Whether he was drowsy and lost track of the subject, and simply caught the word eggs and the number, or whether he intended to be humorous as usual, he nudged me and said in a whisper, "By George, what a fortune, with eggs at twenty-five cents a dozen!" I tell this story not only because it happened, but to remind you that there was once a time when hens' eggs only cost twenty-five cents a dozen.

Before this there was an interesting situation at the meeting of February 7, 1891, when Henry Fairfield Osborn came over from New York and gave a talk on cretaceous mammals. He was showing how a fossil jaw-bone on which Prof. O. C. Marsh had founded a new family, and a part of a skull on which the same authority had founded another family, and some back teeth on which he had founded a genus or some other division, and some front teeth on which he had founded another genus or some other division, all belonged, not to the same skull, but to the same species. I was much embarrassed in the course of this interesting exposition when the door of the assembly room opened and Professor Marsh walked in. I did not know that he was in Washington. As a matter of fact, my embarrassment was not shared by Osborn, who I think rather welcomed a discussion of his paper. But before he finished the door opened again and Professor Marsh went out. Was he angry? I don't know.

Not long ago, at one of our meetings, when General Wilcox presented to the society, for Colonel Roosevelt, a copy of one of the latter's books I told briefly the story of the only appearance before the society of the only naturalist President of the United States. It is worth repeating and perhaps may be made a matter of permanent record. When the program committee of the society was arranging the papers for the meeting of May 8, 1897,

Mr. Roosevelt, then Assistant Secretary of the Navy, had recently published a letter in *Science* criticising C. Hart Merriam's classification of the big game animals of America, not necessarily from the standpoint of the systematic zoologist, but from the viewpoint of a hunter, claiming that Merriam went into too many refined details, and that many of the points known to old trappers and hunters should be taken into consideration in basing a classification. Having this letter in mind, Merriam told the program committee that he would give a talk on big game animals at the May 9th meeting if Mr. Roosevelt could be induced to be present and to discuss the paper. So it was arranged. I was president of the Biological Society at that time, and after the meeting (in the old hall of the Cosmos Club) was opened Mr. Roosevelt and Mr. W. Hallett Phillips came in at the back end of the hall—rather conspicuous because they were the only persons in the room in evening dress—and listened with intentness to Merriam's talk, in which, of course, he riddled Roosevelt's argument in *Science*. It was a long and very interesting address. At its conclusion I invited Mr. Roosevelt, although he was not a member of the society, to take part in the discussion, whereupon he and Phillips came to the front, Phillips sitting in a front seat, and Roosevelt began to talk. He made a very forceful argument from his viewpoint and from that obviously of other hunters, and rather staggered some of the really scientific men in the audience by the cogency of his reasoning. He talked at length, as was customary with him, and the hour of adjournment (10 o'clock) came before he had finished, but by unanimous vote he was allowed to proceed until he was satisfied. He sat down after having made a distinct impression on his scientific and rather critical audience. Merriam asked for five minutes in which to reply, in the course of which he completely demolished the Rooseveltian argument, and there was nothing more to be said. It was a memorable meeting, and no one who was there will ever forget it. Most of us saw Roosevelt for the first time then, and were greatly impressed by him. Among the taxonomists present there were, of course, lumpers as well as splitters, and the lumpers got some satisfaction from the future President's arguments.

It is only rarely that I tell a story in which one of the characters is anonymous, but the circumstances connected with this

one are such that I fear I must not mention the name of a very well known speaker who was invited to address the society during the early nineties, but whose remarks were disappointing in that he started off with a profound introduction and then proceeded to elaborate his thesis in a manner which was characteristic of the man and which by no means upheld his well-founded scientific reputation. The meeting adjourned, and as I passed out of the room, Dr. Theobald Smith, touching shoulders with me, said, "Did that talk remind you of one of the Chinese puzzle boxes?" "No, why?" I said. "Why you must know those trick boxes where you work for a while and finally open it, and then find another box inside, and you work for a while and open that and find another, and you keep on opening boxes until down in the middle there is a very small insignificant box." Of course I was delighted with this, and going into the adjoining room in the Cosmos Club found Dr. G. Brown Goode sitting there sipping from a tall glass. I told him this incident with great joy, and had no sooner completed it than I was horrified because it suddenly occurred to me that the speaker of the evening was a former teacher of Goode. However, I need not have worried, because he smiled his genial smile and said, "I peeked into the room and saw that Blank was getting confidential with his audience, and so came in here and ordered a bottle of beer."

All of these incidents occurred at least nearly 30 years ago. Many strange things, however, have come up in our meetings in more recent times, as for example:

> Extract from Proceedings of The Biological Society of Washington January 20, 1906:
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> Dr. Albert Mann related a case of the capture and raising from the floor of a snake by a spider in Pennington (N. J.) Seminary.

Dr. Mann apparently did not explain what the snake was doing in the seminary! Dr. Mann is always entertaining and scientifically veracious when he talks of Diatoms, and in this story he was surely entertaining.

But enough of these stories. Since those very early days the Biological Society has swollen and cracked and given out satillites, just as did the old Philosophical Society. The entomologists broke away first, then the botanists, and now the Aquarium Society has been founded, and the Naturalists Field Club has

been in existence for a long period, and I can see that the mammalogists, the ornithologists and the herpetologists and the malacologists will, before many years, be founding their own local organizations (the helminthologists already have one). But if they remain satellites and circle around the old parent body we shall not complain. Most of the special workers, however, fail to see the advantage of the broadening out which the meetings of the Biological Society can give them. For myself, I am an entomologist all day long and every day in the week—I am living with entomology. But I am not a biologist unless I not only read along other lines but unless I go to the meetings of this society; and I enjoy it honestly, more than any other society. I like to hear Hay, Jr., talk about his turtles; I enjoy Hitchcock when he lectures on grasses; when Lyon talks bacteria my mind is open; when the elder Hay comes in with a monstrous fossil I listen with the same pleasure as when Palmer exhibits one of his perennial finds along the shores of Chesapeake Bay even if I am somewhat saddened by his pained expression of countenance whenever he speaks in public; and the other Palmer in his new side line of the history of science is intensely interesting, and I leave every meeting feeling perfectly convinced that if I were taken away from Washington one of the things which I should most mourn would be my inability to attend every one of these meetings. I fail entirely to understand why others of the older members of the society have not this same feeling.